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ABSTRACT

Political science and its subfields cannot ignore the work being done in two areas of the life sciences: sociobiology and neurobiology. Current theories of political socialization which suggest that society molds the child will be increasingly affected by sociobiological theory which posits that children operate as independent actors in the socialization process. However, both the social sciences and sociobiology may have to be emended in the light of the emerging field of neurobiology. Leading neuroscientists not only indirectly support the sociobiological contention that children may play an active role in the socialization process, but also they tend to see the brain in emergent, dualistic, and mentalist terms. That is, the mind exists as a separate entity from the physical brain and is not reducible to it. Thus, three implications of neurobiological theory that apply even beyond children and politics to all humans and all behavior must be considered: (1) children are independent actors in the socialization process; (2) self-consciousness, as the child matures, may play an increasingly independently important role in the behavior of the child; and (3) the role of children in the socialization process and of self-consciousness in human (political) behavior must be considered in the light of genetic variability. (Author/KC)

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SOCIOBIOLOGY, NEUROBIOLOGY AND POLITICAL
SOCIALIZATION

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Paper presented at the 1979 Annual Meeting of the
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Each person is an idiom unto himself, an apparent violation of the syntax of the species. . . .

While the child needs and wants love and security, he does not want them to interfere with his impulses, his freedom, or his preferred ways of acting. From the very start of his life he is resistant to the smothering effects of his social environment. . . .

. . . a person who harbors many determining tendencies in his neuropsychic system is freer than a person who harbors few.

Gordon, Allport, Becoming

Scholars, like children, may defy established authority. One such challenge arises from the emergence of biology to become the 'antidiscipline' to the social sciences. As Edward O. Wilson's term graphically suggests and as Wilson emphasizes, an antidiscipline involves "the special adversary relation that exists initially between the studies of adjacent levels of organization" (Wilson 1977 p. 127; Wilson, 1978, p. 7). An antidiscipline exists for every discipline in its early stages of development; so that, according to Wilson, if many-body physics had its particle physics; chemistry its many-body physics; molecular biology its chemistry; cellular biology its molecular biology; now by implication social science is to have its life science. This latter development, states Wilson, has occurred at the present time because "the relevant branches of biology--neurobiology and sociobiology--are only now becoming mature enough to attain a juncture with the social sciences" (Wilson, 1977, p. 127).

In general, such a juncture implies the attempt by the antidiscipline to reformulate and to reduce the subject-matter of the higher discipline to the level of analysis commensurate with the level of the lower. In the particular case of the juncture of the life and social sciences, it means the attempt on the part the former to reduce the latter to the level of its formulations. Or as Wilson put it succinctly in his imperial Sociobiology (1975, p. 4), social science is to be 'biologized.'

Such an attempt is of course necessarily reductionistic in spirit and, as Wilson acknowledges (1977, p. 138; 1978, p. 13), it is not ultimately likely to be wholly successful: the complex and emergent properties that characterize human behavior are not likely to yield to a biological reductionism. These same properties equally resist explana-

tion, however, at the hands of a behavioral science grounded upon an environmental determinism. Ironically, the present theoretical approach of the higher discipline--social science--is often no less reductionistic than that of its would-be antidiscipline, biology. Caught between a present environmental reductionism and a future biological one, where is the thoughtful social scientist to turn?

I do not pretend to know the answer, and I rather doubt that anyone is in a position to know at the moment. Writing this paper, I feel somewhat like the editors of a volume on "Brain and Human Behavior" several years ago (Karczmar and Eccles, 1972) whose very first line of their "Introduction" began, "The disconcerting idea that dawned all of a sudden upon the Editors is that this volume may be premature by about 100 years."

Yet I contend that we as social scientists are forced, whether we like it or not, to rethink our mission; we can no longer fall back on approaches that are coming under increasing challenge and have not, in any case, proven their worth. If the social sciences could point to a demonstrated record of past success, the emerging challenge to 'biologize' them would die aborning; such is surely not the prospect in sight (see Wahlke, 1979).

It is the purpose of this paper--in view of the uncertain and threatening future facing the social sciences-- to explore the implications of those life sciences most relevant to human behavior--sociobiology and neurobiology-- and to do so by focusing on the area of political socialization. The area of socialization in general happens to be one of the few that sociobiology has explicitly and in some depth treated (there is perhaps no area at all in the social sciences that neurobiology presently relates to). The political dimension of the socialization process limits the topic further, thus both making it more manageable while serving my personal interests and background as a political scientist.

This paper will attempt to do the following: (1) briefly describe the present status and approach of political socialization; (2) present the main outlines of sociobiological theory before focusing on a critical discussion of Robert Trivers' analysis of parent-offspring conflict; (3) indicate the increasingly critical role to be played by neurobiology in understanding human behavior and suggest in very general terms the ideas currently held by the leading neuroscientists on the relation of the brain to behavior; and (4) to indicate how the relevant concepts of the contemporary life sciences might increasingly impinge upon the area of political socialization.

I. The Current Status of Political Socialization

One thing may be safely said at the outset concerning political socialization: it has not yet been 'biologized.' Indeed, the field has a strong environmentalistic bias. "The most conventional definition" of the term, Sears tells us (1975, p. 45 emphasis in the original) "implies society's molding of the child to some a priori model, usually one perpetuating the status quo." The possibility of genetic or biological

influences are usually not even considered, although in recent years they may be mentioned in passing and then immediately discounted. Weissberg writes (1974, p. 6), for example: "We assume that each citizen was not born with existing political arrangements in his genes; political dispositions were learned, and this learning is the process of political socialization." Having set up a strawman ("each citizen was not born with existing political arrangements in his genes"), Weissberg can easily dismiss it; but in so doing he ignores the real possibility that different children may react quite differently to the same general (political) environment, thereby implying the critical role biological and quite possibly genetic factors might play in the socialization process. Certainly this possibility deserves to be treated as an open question to be explored rather than one simply resolved by dictum.

Fortunately, alternative views of the socialization process have also been in evidence. The main alternative view, according to Sears (1975, p. 45; emphasis in the original), emphasizes "The child's idiosyncratic personal growth, in which the developing human being gradually attains his own personal identity, which allows him to express himself and to seek to meet his idiosyncratic needs and values in his own way." This Allportian formulation does imply an individual variation in genetic predispositions and capacities; Allport himself (1955) makes these points explicit--in a striking anticipation of contemporary biology.

An implicit acknowledgement of genetic or biological factors at work in the political socialization process may also be observed in White (1968, 1969) Harvey and Harvey (1970) and Jaros (1970). More recently the possible impact of such factors have become openly recognized (White, 1972; Renshon, 1977; Davies, 1977). And the past year has seen the beginnings of an application of sociobiological theory to the field (Schubert, in press; Willhoite, in press; and White, 1978; in press). But in order to see how sociobiological theory might be applied to the field, we must first critically consider what it is.

II. Sociobiological theory and Parent-offspring Conflict

Sociobiology attempts to explain behavior within the context of the evolutionary process. Thus it assumes that an underlying genetic variability characterizes all populations of sexually reproducing animals; that this variability assures as well as influences a wide range of phenotypic traits, including those of behavior; and that invariable shifts in the external environment serve as the basis for the operation of a natural selection wherein some organisms with their particular genes and traits are favored over others. As a result, changes in the frequencies of the genes in a population occur over the generations; those changes indeed define and maintain the evolutionary process.

Sociobiology, however, relates behavior not simply to a general evolutionary perspective, but to one that is distinctly its own. "The central theorem of sociobiology," according to David Barash (1977, pp. 63,81), states that, "When any behavior under study" has some genetic basis, "animals should behave so as to maximize their inclusive fitness," that is, their "net genetic representation in succeeding generations, including other relatives in addition to offspring."

In sociobiological theory the gene becomes the measure of all things; its survival the ultimate life force. Thus Wilson (1975, p. 3) translates Samuel Butler's 19th century adage--a chicken is only an egg's way of making another egg--into 20th century terms: "The organism is only DNA's way of making more DNA." Therefore, "in a Darwinist sense the organism does not live for itself. Its primary function is not even to reproduce other organisms; it reproduces genes, and it serves as their temporary carrier." Human beings are consequently, as Dawkins (1976, ix) puts it, "survival machines--robot vehicles blindly programmed to preserve the selfish molecules known as genes."

Sociobiology traces its intellectual ancestry back to Charles Darwin, but much of its impetus and its very name stem from Wilson's monumental Sociobiology (1975). As I point out elsewhere (White, 1978), selfish genes and individualistic organisms imply a competitive scholarship, so that even a fledgling field is not immune from rivalries and dissent. It is not coincidental, I submit, that the year following the publication of Wilson's opus, two prominent biologists associated with the general approach which Wilson labels sociobiology refrained from mentioning the term save in reference to Wilson's work bearing the name and that only once by one of the authors in the way of a critique (Trivers, 1976; Dawkins, 1976).

If sociobiology is itself far from monolithic, neither is current evolutionary thought. One issue of dispute remains precisely the insistence of sociobiology that the unit of selection in evolution is the gene rather than the individual or the group. Sociobiological thinking arose principally in response to the publication of Wynne-Edwards' Animal Dispersion in Relation to Social Behavior (1962) and its thesis that animals limited the number of their offspring in order to benefit the group as a whole by controlling its population.

The counterargument that in effect developed into the sociobiological approach (W. D. Hamilton, 1964, 1971, 1972, 1975; J. Maynard Smith, 1964, 1966; and G. C. Williams, 1966) maintained that apparently self-sacrificing or altruistic behavior (as defined solely in terms of reproductive success) ultimately was reducible to the attempt by the animal to pass on, if not literally its own genes, replicas of those genes that happened to be shared by others. Now those 'others' are most likely to be close relatives; each offspring, for example, receives half his chromosomes from each parent and therefore shares half of his genes with each--while sharing on the average half his genes with each sibling. Thus it is possible for an animal to sacrifice its own life to save, say three siblings and on a strict cost/benefit analysis to have gained by the personal sacrifice; that is, even if one's personal fitness--the representation of one's own genes in succeeding generation--were diminished, one's inclusive fitness--representation in future generations of all the replicas of one's genes in close relatives--is in fact increased.

If family members share genes in common, however, the other side of the coin is that each member of the family retains his own unique set of genes--multiple identical births aside. In sexually reproducing animals, each organism is genetically unique. Barash (1977, p. 172; emphasis in the original) sets forth the sociobiological implications: "A world in

which virtually every individual is genetically distinct is one in which substantial disagreements between individuals would be expected, with each selected for maximization of its own inclusive fitness. Therefore real possibilities exist for conflict of interest between individuals of the same sex (male versus males and females versus females), between mates (males versus female), and even between parents and offspring."

It is precisely the application of this sociobiological insight to the socialization process that characterizes Robert Trivers' classic analysis of parent-offspring conflict (1974). If parents and offspring share fifty percent of their genes, they also do not share fifty percent. Parents and offspring to that extent, then, have different genetic interests.

Rather than viewing offspring as "passive vessels into which parents pour the appropriate care," Trivers (1974, p. 249) asserts that they must be viewed as independent "actors in this interaction" and that therefore "conflict must be assumed to be at the heart" of the socialization process. The offspring will invariably want more 'parental investment'--love, care and other resources--than the parent is selected to give. Offspring resistance to parental weaning provides an apparently striking example of the resulting conflict.

Trivers (1974, p. 260) summarizes his analysis and applies it to human beings as follows:

. . . a fundamental conflict is expected during socialization over the altruistic and egoistic impulses of the offspring. Parents are expected to socialize their offspring to act more altruistically and less egoistically than the offspring would naturally act, and the offspring are expected to resist such socialization. If this argument is valid, then it is clearly a mistake to view socialization in human (or in any sexually reproducing species) as only or even primarily a process of "enculturation," a process by which parents teach offspring their culture . . . According to the theory presented here, socialization is a process by which parents attempt to mold each offspring in order to increase their own inclusive fitness, while each offspring is selected to resist some of the molding and to attempt to mold the behavior of its parents (and siblings) in order to increase its inclusive fitness. Conflict during socialization need not be viewed solely as conflict between the culture of the parent and the biology of the child; it can also be viewed as conflict between the biology of the parent and the biology of the child.

Trivers' conclusions are sure to prove provocative to the student of political socialization; but he can ill afford to lightly ignore or to discount them. The central premise at Trivers' analysis--that offspring are independent actors, not merely passive recipients in the socialization process--is almost wholly absent from the current political socialization literature. Yet it has a basis that transcends the sociobiological

context in which Trivers places it. A generation ago Allport (1955) recognized the individuality of the child and his active role in the socialization process. If Allport accounts for the individualistic and independent behavior of the child by emphasizing his purposive desire to fulfill his unique potential, such a pre- and asociobiological perspective is not thereby invalidated. As we shall see, current neurobiological thought lends some support to this position. So also does recent work in child and developmental psychology (Jackson and Jackson, 1978). As I have maintained elsewhere (White, 1978), moreover, recent presociobiological evolutionists have also implied an active role for the child in the socialization process. Schubert (1979), moreover, shows that contemporary ethology explicitly recognizes the dynamic interaction of offspring with parents in the socialization process.

If Trivers may therefore overstate the uniqueness of his own position, as well as to understate its larger foundation, he does not overstate its importance. The sociobiological basis for the dynamic role of children in the socialization process may not, furthermore, be necessarily more valid than alternative perspectives. Although I have touched upon most of the following five criticisms of sociobiological theory elsewhere (White, 1978; White, in press), I will briefly outline them here:

1. Sociobiological theory has a reductionistic tendency. If behavioral science is environmentalistic, sociobiology reverses the emphasis: As the antidiscipline to the social sciences, it tends to reduce behavior, as Wilson acknowledges (1977), to the level of the gene. Yet as Wilson goes on to conclude (1977, p. 138; 1978, p. 13):

The urge to be a reductionist is an understandable human trait. Ernst Mach captured it in the following definition: "Science may be regarded as a minimal problem consisting of the completest presentment of facts with the least possible expenditure of thought." This is the sentiment of a member of the antidiscipline, impatient to set aside complexity and get on with the search for more fundamental ideas. The laws of his subject are necessary to the discipline above, they challenge and force a mentally more efficient restructuring; but they are not sufficient for its purposes. Biology is the key to human nature, and social scientists cannot afford to ignore its emerging principles. But the social sciences are potentially far richer in content. Eventually they will absorb the relevant ideas of biology and go on to beggar them by comparison.

2. Sociobiological theory has a functionalist bias. "A fundamental assumption of sociobiology," writes Barash (1977, p. 33) "is that behavior patterns are in fact adaptive." Ironically, Barash (1977, p. 374) concludes the same work by speculating that our evolutionary-based behavior as humans may in fact in the contemporary world constitute "a serious threat to ourselves as a species."

Well over a generation ago J. B. S. Haldane (cited in Hardy, 1965, p. 10) pointed to the fallacy that the action of natural selection must

make a species fitter in relation to the environment. If, after all, 99 percent of the species that have ever existed have become extinct (Handler, 1970), then clearly one cannot safely assume that empirically observable traits are necessarily optimally adaptive (see Lewontin, 1978).

This basic criticism of sociobiological theory applies as well to functionalist assumptions within the behavioral sciences. In both cases, functionalist theory dictates empirical conclusions which may be at variance with the real world, and in both cases the scientist finds it more difficult to adjust the theory than to recognize the reality.*

3. Sociobiological theory has a typological tendency. According to Ernst Mayr (1963, pp. 5,6), "The replacement of typological thinking by population thinking is perhaps the greatest conceptual revolution that has taken place in biology." The populational thinker, Mayr states, "stresses the uniqueness of everything in the organic world" and the fact that for all species including our own "no two individuals are alike," while the typologist emphasizes the average or 'normal' type of organism in a larger population rather than the range of individual differences or variation within the population.

Sociobiological theorists of course fully acknowledge the reality of human variability; Trivers, for example, has observed that, "the genetic system not only produces the genetic variability that natural selection works on but is itself a consequence of natural selection" (1976, p. 62). But his theoretical concerns remain implicitly typological, that is, focused on building a systematic science that stresses universal patterns. As a result, Trivers can explicitly recognize that 'the genetic system' operates in such a way as to assure 'genetic variability', without fully appreciating the necessary consequences of such a process.

* Functionalism theory within the behavioral sciences is based, as Dubos observes (1972, p. 240) on the biological concept of "homeostasis" popularized by Walter B. Cannon and influenced by Claude Bernard. Yet as Dubos goes on to point out (1972, pp. 249-248): "The cybernetic processes of responses operating through self-correcting feedbacks provide powerful and practical mechanisms of automatic control, but they are effective only as long as conditions remain fairly stable. This limitation, however, has often been ignored by biologists, ecologists, and sociologists, with the result that belief in the efficacy of homeostatic processes has tended to create the impression that all is for the best in the best of all possible worlds. The very word homeostasis seems indeed to imply that nature in its wisdom elicits responses that always bring the system back to original condition. For example, there is no mention of disease in Cannon's Wisdom of the Body, as if homeostatic negative feedbacks in physiological processes were always successful in preventing the harmful effects of environmental influences and in assuring healthy development. But this is far from the truth. In fact, homeostatic processes that appear to be successful because they exert a protective or reparative function at the time they occur commonly elicit malfunction at a later date. . . . The wisdom of the body is often a short-sighted wisdom."

As George Gaylord Simpson (1964) maintains, the fact that universal processes in life so operate as to produce change and diversity, as in precisely the case of 'the genetic system,' means that the life sciences are historical in nature (in every sense of the latter term) and its subject matter, including evolution itself, importantly unique, irreversible over time, and unpredictable.

These conclusions, as applied to Trivers' analysis of parent-offspring, translate as follows: Parent-offspring conflict may well constitute a basic and universal pattern for all sexually-reproducing species, but the working out of this pattern implies a myriad of conflict situations brought about by a wide range of unique genetic and historical factors. The operation of a universal pattern of inter-generational conflict, moreover, implies disruptive discontinuity and therefore suggests that specific historical predictions as to the outcome of the conflict are quite problematic. Trivers simply does not concern himself with the question of the historical and hence unique manifestations of his universal principle; but his preoccupation with the theoretical and the typological means that he evades precisely those questions closest to the concerns of the social scientist.

4. Sociobiology is, for the foreseeable future at least, nonoperational. Sociobiological theory assumes that a cost/benefit analysis in evolutionary survival terms is feasible.* But note that any such analysis also assumes that we know the relationship between specific behavioral acts and future reproductive success. Woody Allen may be wrong in supposing (in Sleeper 1973) that in some future society eating steaks, mashed potatoes, and fudge may actually be wholesome and therefore adaptive behavior--but who can be sure? The time was when bleeding was considered curative; and even today the critic Ivan Illich (1976) argues that "the medical establishment has become a threat to health." The position Illich takes is admittedly extreme, but Walsh McDermott, (1977, p. 154) long a professor of public health and medicine at Cornell, points out how problematic even now is the connection between health care and health: "At present we are far less capable of approaching measurements of health care effectively than we think." According to McDermott (1977, p. 156 emphasis in the original), "it is essential to realize that the particular approaches used today of attempting direct evaluations of physician performance and validation of his technologies contain serious defects and may well prove to be failures."

If the effectiveness of contemporary medical practices remain in question, how valid can the assessment be, in cost/benefit survival terms, of any other area of human life? Trivers (1974, p. 262), for example, acknowledges that parent-offspring conflict may result "ironically because of an overlap in the interests of parent and young," that

* It is ironic that sociobiological theorists should employ for their individualistic perspective an economic method that is favored, according to E. J. Mishan (1971, p. 6), over profit and loss accounting precisely because "in cost-benefit analysis we are concerned with the economy as a whole; with the welfare of a defined society, and not any small part of it."

is, when parents, with greater experience, anticipate harmful consequences that their children fail to see. A clearcut case would appear to be the parent who hauls off his resisting two year old from the onrush of traffic in a busy street. Yet how many situations are so clear-cut? If both parent and adolescent offspring argue over a course of action and both insist that they are acting in their best interests, how does one decide who is right in survival terms?

This question is further complicated by the fact that the sociobiological and the common sense basis for selfish and altruistic behavior may strikingly diverge. As Gunther Stent (1977) observes, for sociobiology altruistic and selfish behavior is defined solely in survival terms, whereas in everyday language, such behavior has an important motivational basis. Thus if a child willingly shares candy, drugs, or cigarettes with his peers, his is clearly altruistic behavior in an everyday sense, but presumably not in sociobiological terms. Indeed, if the items in question are in fact deleterious to one's health and therefore survival chances, then a selfish refusal to share them would, again in sociobiological terms, really be altruistic--and vice versa.

The larger problem remains, quite simply, that no method exists for determining the costs or benefits of various behaviors. If sociobiological theory is valid, especially as applied to humans, its validity has--and for the foreseeable future can have--no firm empirical foundation.

5. Fifth and finally, sociobiology has a mechanistic bent. Dawkins (1976, p. 157) puts it starkly: "A body is really a machine blindly programmed by its selfish genes." I have already attempted to show elsewhere (White, in press) that Trivers fails adequately to allow for the influence of developmental and cognitive factors in treating parent-offspring relations. This failure adequately to consider the independent role of cognitive factors was characteristic as well of Wilson in his ground breaking Sociobiology, as so forcefully and critically pointed out by the late C. H. Waddington (1978).

Yet Wilson in his Sociobiology also explicitly anticipated the critical future role of the neurosciences in accounting for human behavior; thus he concluded his first chapter by writing that, "Although behavioral biology is traditionally spoken of as if it were a unified subject, it is now emerging as two distinct disciplines centered in neurophysiology and in sociobiology, respectively." According to Wilson (1975, p. 6), "Whole patterns of animal behavior will inevitably be explained within the framework" of the emerging neurobiology.

But then if sociobiology is, in Wilson's words (1975, p. 4), "the systematic" study of the biological basis of all social behavior," is it not premature, since clearly the neurobiological basis for animal behavior has not yet materialized. What Wilson does is anticipate that basis by assuming that it will be entirely compatible with current sociobiological theory, an assumption that may not in fact be tenable. Wilson begins Sociobiology, for example, with the assertion "that self-knowledge is constrained and shaped by the emotional control centers in the hypothalamus and limbic system of the brain," which "flood our consciousness with all the emotions . . . that are consulted by ethical

philosophers who wish to intuit the standards of good and evil" and which, having evolved through natural selection, must therefore in its functioning manifest the operation of sociobiological principles. As Wilson directly puts it in his On Human Nature (1978, p. 2), "The brain exists because it promotes the survival and multiplication of the genes that direct its assembly. The human mind is a device for survival and reproduction, and reason is just one of its various techniques."

Wilson, then, anticipates future neurobiological research by assuming it to be fully compatible with sociobiological theory. In this respect, Wilson is similar to B. F. Skinner (1971, p. 186) who, in denying that his behaviorism implies 'an empty organism', states, "A great deal goes on inside the skin, and physiology will eventually tell us more about it. It will explain why behavior is indeed related to the antecedent events of which it can be shown to be a function." (See Searle, 1978; Grene, 1978; Fuller, 1978; and Frankel, 1979, esp. pp. 43-45). Wilson and Skinner clearly have differing expectations of the findings of future neurophysiological research and both cannot be right.

Wilson (1978, p. 75) not only anticipates future neurobiological findings; he also evaluates models of the human brain by how closely they adhere to his sociobiological principles. Thus if too simple a neurological approach is to be avoided, so also "too complex a neurological model can lead back to a vitalistic metaphysics." Since the latter model leads to conclusions theoretically unacceptable to Wilson, "a compromise solution" is advocated.

The very real possibility exists, however, that such efforts to bind future brain research to present sociobiological (or behavioral) theory is premature and unrealistic. Even sociobiological theory may not be fixed at this point. Wilson himself (in Davis and Flaherty, 1976, p. 11) has raised the possibility "that a wholly new picture of organic evolution may be about to emerge, and that this may be one of the turning points in the history of science." If so, Wilson's present speculations on the science of the brain would quickly become dated. But his speculations might well become dated in any case--for the neurosciences will no doubt develop in their own way and, to paraphrase Wilson, "a wholly new picture" of the human brain "may be about to emerge," and "this may be one of the turning points in the history of science."* Even if no one, including this writer, is in a position to predict the course of this development, neither can anyone dealing with human behavior afford simply to ignore the critical role of the brain. Let us, then, at the very least consider that role within the context of contemporary neurobiology.

* The present sociobiological emphasis on ultimate or evolutionary causes of behavior assumes that consciousness is determined by a mechanistic process of self-selection. It does not allow for the possibility that consciousness itself, in higher animals at least, may play an active role in the evolutionary process; that is, evolutionary or ultimate causes may include consciousness as a casual factor. See Hardy (1965), Searle (1978) and Griffin (1978).

III. Neurobiology

One scientific frontier remains. Certainly this appears the case within the life sciences. As Gunther Stent (1969, p. 71) writes:

There now seems to remain only one major frontier of biological inquiry for which reasonable molecular mechanisms still cannot be even imagined: the higher nervous system. Its fantastic attributes continue to pose as hopelessly difficult and intractably complex a problem as did the hereditary mechanism a generation ago. And so increasing numbers of veteran molecular geneticists are now turning toward the nervous system, in the hope that its study may soon enter a romantic phase, similar to that which attended the birth of molecular genetics. It seems likely that in the coming years students of the nervous system, rather than geneticists, will form the vanguard of biological research.

If the future course of the neurosciences will eventually relate to human behavior, that day, alas, may take centuries to arrive. As mentioned earlier, Karczmar and Eccles in their 1972 volume on 'brain and human behavior' are struck with the "disconcerting idea" that the "volume may be premature by about 100 years," for "we are still at the primitive stage in the development of our knowledge of 'brain and behavior.'"

What, then, is the social scientist to do? Four choices are, I believe, open to him. First, he can simply ignore (or try to ignore) the problem. Secondly, he can assume future findings in neurobiology to fit his preexisting theoretical framework, just as Wilson and Skinner have done; thirdly, he can attempt to incorporate the technical knowledge now emerging from the neurosciences into current social science theory, perhaps qualifying the latter as it proves necessary; and fourthly, he can try to ascertain the more basic views of the brain that are emerging based upon the work of the leading authorities within the field. Let us briefly consider each of these alternatives in turn:

The social scientist has already shown himself to be adept in adopting the first solution: that of ignoring a field which appears both remote and complex. Only within the past decade or so have the emerging fields of ethology and behavior genetics been related to political science. See Somit (1976); Masters (1975); Corning (1971) and Willhoite (1976). Yet ignoring a relevant area of study provides no viable solution. It means in reality falling back on the conventional wisdom within one's own field, whose assumptions may nonetheless prove open to question in the face of the newly emerging areas. And of course it may be for precisely this reason that so much inertia and resistance can be found to these newer approaches. But current behavioral theory will not be salvaged by ignoring unpleasant developments elsewhere; and, more to the point, it will not account for human behavior if its basic premises turn out to be untenable in the light of neurobiological research.

The second choice, that of presuming future findings to prove compatible with existing theory, is unacceptable for largely the same reason. It is much more likely that present views of behavior (including much of sociobiology) will have to be qualified in the light of an emerging neuroscience rather than vice versa.

The third possibility will in fact eventually materialize, that is, the new knowledge of the brain will be incorporated within the social sciences. Even now the existing new developments in the biochemistry of the brain and in psychopharmacology demand attention by social scientists, if only on the level of public policy. But a systematic incorporation of neurobiological knowledge into the social sciences is still premature.* The former area is still too new, changing and unfinished to be ready for such a synthesis, while the latter is still too unreceptive and unprepared to receive it. And even if both fields were ready, the undertaking would be an imposing one. If Sinsheimer (1971) is correct in saying that the human brain may not be fully capable of comprehending itself, then the task of fully comprehending the relationship between the brain and behavior may be even more difficult. And since Sinsheimer is implicitly assuming the best scientific minds around, those of us with lesser faculties find ourselves in an even more untenable position. The rampant specialization within the sciences may well have a genetic and neurological basis: How many renaissance minds are or will be available to undertake the task of synthesizing neurobiology, sociobiology and social science? I wish Edward Wilson a long life, but I freely confess my own inadequacy to the task.

The final position involves relying on the more general and accessible writings of the acknowledged leaders in neurobiology and relating these to the social sciences. I find this choice the most congenial. It is realistic (it can be done); it is somewhat tentative (as it should be, given the inevitability of later developments); and it is sensible in scientific terms (it means correcting positions even now untenable in the light of current neurobiological theory). I believe these conclusions apply across the board to behaviorist psychology, to sociobiological theory and to behavioral science.

Accordingly, I will proceed to examine the basic thought of the following outstanding neuroscientists: John Eccles, R. W. Sperry, Wilder Penfield and Karl Pribram. Two points should be made at the outset: First, like any group of individuals, scientists or otherwise, these students of the brain do not necessarily agree with one another on any given point--but, as it happens, they do form a consensus on those questions most relevant to the study of human behavior. Secondly, the fact of their forming a consensus does not assure the validity of their position or that, even if they should prove to be basically right, that that position will not be importantly qualified or refined in future years. Nonetheless, those of us who are not authorities in the area

*For an albeit premature but nonetheless strikingly prophetic attempt to link psychopharmacology with political behavior, see Somit (1968). For a review of recent discoveries in research on the brain, see Restak (1979).

should ask ourselves one simple question: Upon what basis should we substitute our judgment for theirs, especially if our judgment is dictated by the demands of a theoretical approach whose own validity is at best problematic?

The neuroscientists I have mentioned agree, for the most part, on three important points that are of importance to the social sciences: First, with one exception, they hold to a dualist rather than a monist position with respect to the mind-body problem; secondly, and of most direct concern to the social sciences, they all agree that human consciousness acts as an important causal agent in its own right in the realm of human behavior; and thirdly, these first two views mean a rejection of reductionism in any form as applied to the human brain. Let us now consider these positions in turn.

1. With the one exception of Pribram, the remaining neuroscientists (Eccles, Sperry and Penfield and, for the matter, the great pioneer in neuroscience during the first decades of the century, Charles Sherrington) take a dualist position; that is, that the mind exists as a separate entity from the physical brain and is not reducible to it. They thus take precisely the position that Wilson (not to mention Skinner) would wish to avoid: a model that "can lead back to a vitalistic metaphysics, in which properties are postulated that cannot be translated is to neurons, circuits, or any other physical units." (Wilson, 1978, p. 75). Yet Eccles (1976, p. 160) views the materialist monism that corresponds to Wilson's position "as an ancient superstition according to which man is the victim of iron determinism as defined by nineteenth-century physics."*

* Wahlke (1979), in his presidential address to the American political science Association, makes the dubious assumption that knowledge newly emerging from the life sciences can be added to political science without any qualification of the underlying methodological principles of its present behavioral approach. These typological and materialistic principles, however, can not be considered immune to the influence of differing approaches emanating from the life sciences. In particular, Wahlke's anti-mentalistic stance should be reconsidered in the light of contemporary neurobiological theory. Indeed, his position runs counter to the recent re-discovery among at least some behaviorist psychologists of introspection as a valid method of attaining psychological knowledge. See, for example, David Lieberman (1979, p. 332), who concludes a recent article entitled "Behaviorism and the Mind: A (Limited) Call for a Return to Introspection" with the words of the biologist Seymour Kety:

[To deny] the existence or the importance of mental states merely because they are difficult to measure or because they cannot be directly observed in others is needlessly to restrict the field of the mental sciences and to curtail the opportunities for the discovery of new relationships. The remarkable hallucinogenic properties of lysergic acid diethylamide (LSD) are barely hinted at in behavior, and the behavioral disturbances in schizophrenia are a mere fragment of the entire picture. Nature is an elusive quarry, and it is foolhardy to pursue her with one eye closed and one foot hobbled.

I do not intend here an extended discussion of the mind-body problem. It is a problem that I do not fully comprehend, and that, in any case, will remain an open question for the foreseeable future, for, as Penfield writes (1975, p. 114), "a final conclusion . . . is not likely to come before the youngest reader" of his 1975 work dies. Yet, as Wilson rightly recognizes, the larger theoretical model of the brain has direct implications for the study of human behavior; and, as we shall see, a dualist or mentalist perspective suggests that the conscious self will play an active role in one's behavior.

A materialist monism, however, need not imply the complete absence of free choice in human behavior. Wilson's own materialistic 'compromise' model of the brain (1978, p. 77), for example, nonetheless allows for a limited human freedom. Let us explore this possibility further in the monistic though nonmaterialistic position of Karl Pribram. In Pribram's view (1971, 1976), it is "the basic function of the brain to generate the roles"--the languages which "are the key to the structure of consciousness"--by which information (which "is not the property of any single event, but the property of the relation between them") is communicated (1976, p. 301).

Pribram acknowledges that "many biologists, including Sir Charles Sherrington, Wilder Penfield, Sir John Eccles, and Roger Sperry, are dissatisfied with this sort of explanation because they cannot as yet visualize a brain mechanism that readily transforms nerve impulses into subjective experience" (1976, p. 300), but Pribram also makes clear that his own reductive approach does not deny the reality or efficacy of conscious awareness:

. . . we ask not how brain and consciousness interact, but how the organization of interaction of basic brain elements differs in the states characterized by automatisms and those characterized by consciousness. As noted already, this form of reduction is not a pernicious reductionism that denies reality to consciousness or "explains" all the manifestations of consciousness in brain terms. Conscious awareness is a realization as real as is brain. In understanding the origins of the organization of consciousness we employ reductive procedures leading to the structure of brain, but in understanding the organization of brain we employ procedures that are equally reductive and which lead to the structure of awareness. And who is to say that one of these reductions is more fundamental than the others? Or who would claim that these reductions provide the total panorama of the realities we call "consciousness" and "brain"? (Pribram, 1976, p. 303).

Pribram's position, in short, is not, as he explicitly states, "a reductive materialism." Rather, it affirms that scientists are "ready (and capable) to defend spirit as data" while anticipating that "the days of the cold-hearted, hard-headed technocrat appear to be numbered" (1976, p. 312). As Grover Maxwell (1976, p. 312), responding to Pribram's position, concluded: "Pribram has emphasized that consciousness is

important, practically important, even. It is very heartening to hear this coming from a tough-minded scientist. He comes to grips with Ryle's "ghosts in the machine" and comes to the conclusion that ghosts (of the kind that rile Ryle) really exist, and that they too are important."

2. Consciousness, then, may serve as an independently important causal agent in human behavior. Sperry especially stresses this view. Sperry's model of the brain is emergent and mentalist. It is emergent in insisting that the functioning of the brain as a whole is not reducible to its discrete molecular parts, but rather must be treated on its own higher level terms. It is mentalistic in asserting that "those over-all organizational forces and dynamic properties of the large patterns of cerebral excitation" are "correlated with mental states or psychic activity" and that in this brain model, "the causal potency of an idea, or an ideal, becomes just as real as that of a molecule, a cell, or a nerve impulse. Ideas cause ideas and help evolve new ideas." (Sperry, 1965, pp. 86-82.)

Sperry concludes "that it is possible to see today an objective, explanatory model of brain function that neither contradicts nor degrades but rather affirms age-old humanist values, ideals, and meaning in human endeavor." (Sperry, 1965, p. 92); and that, "Considered broadly, our present interpretation goes far to restore to human nature the personal dignity, freedom of choice, inner creativity, and other humanistic attributes of which it has long been deprived by the behavioristic and materialistic movements in the brain-behavior sciences" (Sperry, 1976, p. 176).

Penfield also emphasizes the active and independent role of human thought. His treatment of the Jamesian 'stream of consciousness' metaphor as "a river, forever flowing through a man's conscious waking hours" is instructive. For Penfield finds this metaphor misleading: "A river of water cannot be altered by the man on the bank. But thought and reason and curiosity do cause the stream of consciousness to alter its course and even change its content completely. The biological stream that is hidden away in each of us follows the command of the observer on the bank. . . . It is the mind (not the brain) that watches and at the same time directs" (Penfield, 1975, p. 49).

Similarly, Eccles considers free will, by which he means "the ability to carry out actions that have been planned in thought, or at least to attempt to carry them out" to be "a fact of experience" (1972, p. 217). An assertion of free-will, as Sperry points out, does not rule out the impact of genetic and environmental factors: ". . . the kind of brain visualized here does indeed give plenty of free will, provided we think of free will as self-determination. To a very real and large extent, a person does determine with his own mind what he is going to do from a large number of possibilities. This does not mean, however, that he is free from the forces of his own decision-making machinery . . . nor does it free him from his inherited makeup or his lifetime memories" (Sperry, 1965, p. 87).

3. These emergent brain models are not reducible in materialistic, genetic or environmentalist terms. If, as we have seen, Pribram's position is not a narrowly reductionist one, the neo-Allportian views of Sperry, Eccles and Penfield clearly are not. Sperry, for example, argues that "the forces operating at subatomic and subnuclear levels within brain cells are molecular-bound, and are superceded by the encompassing configurational properties of the brain molecules in which the subatomic elements are embedded" and that, "In the same way the properties of the brain molecules are enveloped by the dynamics of cellular organization, and the properties of our brain cells are in turn superceded by the larger network properties of the circuit systems in which they are embedded." Since "at the apex of the brain's organizational hierarchy are found the large cerebral processes that mediate mental activity" and since "these top-level systems' properties supercede those of the various subsystems they embody," a materialistic reductionism is not warranted (Sperry, 1976, p. 168). Indeed, Eccles has argued that, "The subtlety and the immense complexity of the patterns written in space and time by this "enchanted loom" of Sherrington's and the emergent properties of this system are beyond any levels of investigation by physics or physiology at the present time . . . and perhaps for a long time to come" (Eccles, 1976, p. 117).

An environmentalist reductionism is also, needless to say, out of the question. ". . . the evidence available today, Sperry states (1965, p. 91), "says that we should renounce, along with other aspects of the behaviorist, materialist approach . . . the old Pavlovian-Watsonian conditioned-reflex theory of the psyche, with its radical environmentalism that used to tell us that literally 99 percent of human nature and mind is a product of experience and training."

Eccles (1972, p. 223), responding directly to the behaviorism of B. F. Skinner, writes, "I believe that as human beings we have freedom and dignity. Skinner's theory and the technique of operant conditioning were developed from his experiments on pigeons and rats. Let them be the beneficiaries!" As the late E. H. Lenneberg (1973, p. 128) insisted, "A nervous system is not like a trumpet into which the environment can blow and produce a tune. Brains are not passive conveyors of information. They are very active objects . . . That is why treating the brain as a communication channel or viewing behavior simply as a function of the input to the system is misleading."

Neither is the brain wholly determined by its genetic program. The brain, writes Eccles (1972, p. 175), "is not a fixed action structure. At the higher levels of the brain, modifiability is the essence of its performance, as is evidenced by learning and memory": Our "personal uniqueness and all aspects of its associated experiences are dependent upon the brain; yet it is not entirely dependent on the genetic instructions that built the brain in the manner that is still only vaguely understood."

A phylogenetic reductionism, finally, is questionable; Paul MacLean (1973, p. 58), in his "triune concept of the brain and behavior", does not, like Wilson, simply reduce the higher and later levels of the brain to the earlier and more primitive. The 'neomammilian' brain does not

merely serve the 'reptilian,' even if the first layer may still influence the latest. Citing an earlier work of his, MacLean writes that, "In the complex organization of the phylogenetically old and new structures . . . we presumably have a neural ladder, a visionary ladder, for ascending from the most primitive sexual feeling to the highest level of altruistic sentiments! . . . It is possible that these large evolving territories of the brain are incapable of being brought into full operation until the hormonal changes of adolescence occur. If so, it would weigh heavily against the claims of those who contend that the personality is fully developed and rigid by adolescence, if indeed not by the use of five or six." And not, therefore, by birth or conception.

Now if consciousness operates as an active force in its own right, we still must delimit its range of applicability. Does consciousness, for example, characterize all matter, all life, all species, all humans--of all ages? In other words, we must place consciousness within the context, in Wilson's words (1978, p. 15), of "a planet of staggering organic diversity." And as Sperry (1976, p. 171) notes, there is a comparable diversity of views seriously entertained by scientists--from the panpsychism of Whitehead and C.H. Waddington (even rocks have consciousness) to the position of Eccles who would deny conscious experience, not only to rocks and plants, but even to the minor hemisphere of the human brain.

Now if Sperry, whose "own inclination is to see consciousness as being united in the normal brain" of man (1976, p. 171), differs from Eccles' restriction of consciousness to the left or dominant hemisphere of the brain, they both restrict its presence in man. For Eccles, one should make clear, the restriction really applies to self-consciousness and not consciousness more generally; that is, Eccles would not deny unself-conscious consciousness to animals (Eccles, 1976, p. 334).

Eccles, however, is clear on his restriction of self-conscious thought for humans: ". . . dualists as myself are accused of human narcissism. If this accusation means that in the biological world only human beings are endowed with a self-consciousness, and with a cultural creativity, and that they are distinguished completely from animals by the ability to think logically, creatively, and imaginatively and to communicate these thoughts in every medium of cultural expression, then I am happy to admit that I am guilty of human narcissism" (1976, p. 157).

Yet, interestingly, since Eccles (1972, p. 220) places "the dawn of self-consciousness" as "at least 100,000 years ago," he is less restrictive than Jaynes (1976). Interestingly also, while both Sperry and Eccles appear not to differentiate among humans with respect to self-consciousness, if for no other reason that they generally ignore the question as to whether self-consciousness is a universal human quality, Wilson, (1978, p. 41-43) who otherwise emphasizes universal human traits, explicitly refers to 'noncultural retardates,' who clearly lack human self-consciousness and whose condition, Wilson suspects, may be genetically based. Eccles (1972, p. 222) does, however, state "quite categorically that brains are built by the complex processes resulting from genetic endowment and all the secondary instructions deriving therefrom in fetal life" and that consequently, "Geniuses are born, not made."

Finally, the question that is most central to the concerns of a paper on socialization: Is self-consciousness differentiated by age? According to Pribram (1976, p. 305), "There is a good deal of evidence that self-awareness is achieved gradually and that it is relatively fragile" and that, "based upon the work of Piaget, it is not attained within children before the age of 7 or 8."

It is perhaps ironic that a leading contemporary neuroscientist should rely upon the work of Piaget in order to estimate the age at which self-awareness is attained; for, as Karczmar (1972, p. 9) points out, Piaget's writings "are basically 'extra cerebral,' i.e. not based on the study of the brain as such." The emerging field of developmental neurobiology will in time likely encompass and qualify Piagetian developmental psychology.

Such a future development, however, will not necessarily mean that Piaget's formulations will be reduced to genetic, materialistic or any other lower level explanation. Piaget's dynamic, emergent approach does not, in these large terms, contradict present neurobiological theory, nor would his strictures (1972, p. 376) against too narrow a hereditarian perspective: "In a word, many factors come into play between the possibilities opened up by heredity and the realization of these possibilities, and this is true to a greater extent, of course, the further away we get from elementary stages of development."

IV. Neurobiology and Political Socialization

What, then, are the implications of neurobiological theory for political socialization? Two major conclusions suggest themselves: (1) children (like adults) function as active and independent agents; and (2) self-consciousness, in adolescent children at least, may play an independently important role in (political) behavior. Finally, a third consideration should be discussed as providing a context for dealing with these first two, namely the inevitable presence of individual differences based upon a profound human genetic variability. In treating these three points, their more general applicability should be noted, that is, if they are at all valid, they apply beyond children and politics to all humans and all behavior:

1. Children are independent actors in the socialization process. Here Gordon Allport, Jean Piaget, Robert Trivers and the contemporary leading neuroscientists would all appear to be in agreement. Behaviorist psychology and behavioral social science here contend against third-school and genetic psychology, sociobiology and neurobiology. The former insists on a socialization process that is in effect not a process, the child being passively molded by his environment. The latter areas agree in stressing the child as a force in his own right in reacting against the environment and perhaps in even taking the initiative to influence and change that environment.

On this latter view, learning becomes a dynamic process. The child does not learn merely by narrowly associative means, as S-R learning suggests. As a result, the child will not necessarily become the political mirror-image of his cultural milieu, in effect stamped out by

his culture--as Allport (1955, p. 7) metaphorically describes as "though the impress of a cookie cutter". Children rather will select and qualify the information that comes their way, at times seeking or rejecting it, with the result that social and political continuity is anything but assured. As I have elsewhere argued (White, in press), generational change is inevitable and irreversible for every aspect of social and political life.

The dynamic learning process assumed here will no doubt eventually be related to neurobiology. Indeed, some works already attempt a more general connection (see Teyler, (ed.) Brain and Learning, 1978). But such pioneer efforts represent only the first necessary steps, hardly the last definitive word. As Eccles (1922, p. 3) reminds us, "We are still at a very early stage of our attempt to understand the brain, which may well be the last of all the frontiers of knowledge that man can attempt to penetrate and encompass. I predict that it will occupy hundreds of years into the future."

2. Self-consciousness, as the child matures, may play an increasingly independently important role in the behavior of the child. On this point, behaviorist psychology, behavioral science and sociobiology stand against third-school psychology and neurobiology. The former discount or minimize the role of consciousness thought. Even Trivers' view of offspring as independent actors does not imply conscious thought inasmuch as he is referring to offspring of all sexually reproducing species. But even his discussion of human children does not provide a role for self-awareness. Rather, Trivers implicitly assumes Dawkins' position (1976) that humans, like other animals, are blindly programmed through the genes. For the behaviorist, on the other hand, humans, like other animals, are blindly controlled by the environment. Either way, conscious thought plays no important role in behavior.

Allport and Piaget, in agreement with contemporary neurobiological theory, allow such a role. Piaget's fourth and final stage of intellectual development assumes that the child, as he reaches age 11 or so, can conceptualize his environment and thereby transcend it. This possibility also assumes that learning may be conceptual as well as associative. As I have suggested elsewhere (White, 1969), conceptual learning allows for (though by no means assures) a greater mastery over the environment. The political implications of conceptual learning on the part of children would thus include the probability that older and brighter children, i.e. those children most likely to engage in conceptual learning, will attain a higher degree of political efficacy than other children, as appears to be the case (Hess and Torney, 1967; White, 1968; Harvey and Harvey, 1970).

3. The independently important role of children in the socialization process and of self consciousness in human (political) behavior must be considered in the light of genetic variability. As I have suggested elsewhere (White, in press), Trivers' general statement that children are independent actors needs to be placed within the context of individual differences in dominance that may have some genetic basis. One might, for example, consider the following remark of Malcom X (1965, p. 8) from this perspective: "I learned early that crying out in

protest could accomplish things. My older brothers and sister had started to school when, sometimes, they would come in and ask for a buttered biscuit or something and my mother, impatiently, would tell them no. But I would cry out and make a fuss until I got what I wanted." In other words, Malcom X may have learned that assertive behavior paid off, but he also may have been genetically endowed with stronger propensities to act accordingly than his siblings.

Similarly, the references by E. O. Wilson to 'noncultural retardates' and by Eccles to genius may be linked, as in fact they are by both authors, to genetic influences, thereby suggesting a wide range of genetically influenced individual differences that affect the nature of the brain and consequently behavior.* As Delgado (1977, pp. 4-5) writes concerning research on neurobiological diversity: "... behavioral responses are determined by a combination of genetic factors, environmental circumstances, and individual experiences and all these elements may differ from subject to subject. The deterrent in the investigation of individuality is the complexity and lack of knowledge about the many elements involved. In spite of these differences, with suitable experimental design some aspects may be investigated, and this trend could be important for behavioral neurochemistry."

It is not perhaps surprising that neurobiology in its pioneer days abounds in typological references to 'the' human brain. But, as Delgado suggests, neurobiology may be on the verge of important discoveries in an emerging focus on individual differences. And as Bovet (1972, p. 325) writes, "Today it is possible to suggest the large biochemical differences, evident between individuals belonging to the same species, may represent the basis of their extreme behavioral variability."

* Egalitarians understandingly have difficulty in fully acknowledging genetic diversity and its behavioral implications because of their correct suspicion that human inequality may in part result from such variability. Third school psychologists and the democratic theorists that they have influenced, as I have discussed elsewhere (White, 1975; White, 1979) have precisely this difficulty. Yet it is noteworthy that one of the most prominent of third-school psychologists, Abraham Maslow, in his last writing, the preface to the third edition of his influential Motivation and Personality (1970, pp. xvii-xix) forthrightly faced this issue (rather unlike most of his followers): "The great advances of the last decade or so in the sciences of genetics has forced us to assign somewhat more determining power to the genes than we did fifteen years ago."

"... I must point out that we shall have to prepare ourselves in principle for the shaking consequences of giving up the alibi of social injustice. The more we continue to reduce social injustice, the more we shall find this replaced by "biological injustice," by the fact that babies are born into the world with different genetic potentials. If we get to the point of giving full opportunity to every baby's good potentials, then this means accepting poor potentials as well. Whom shall we blame when a baby is born with a bad heart, or weak kidneys, or with neurological defects. If only nature is there to blame, what will this mean for the self-esteem of the individual "unfairly treated by nature itself?"

In a similar vein, Childs (1975, p. 119), refers to "the fermenting field" of investigating "the extent of polymorphism of brain tissue." In discussing the wide range of biochemical variation in human brains, Omen (1975, p. 101) observes, "It is very likely that the remarkable ranges of behaviors we observe in normal people have their counterpart at the metabolic and physiological level."

If a general connection between genetic, neurobiological and behavioral diversity becomes established, clearly psychological, social, and political phenomena will all have to be treated accordingly. Toulmin's critique of Piagetian psychology (Toulmin, 1972) as being unduly typological and therefore unmindful of genetically based individual variation may thus receive empirical support. In the same way, my earlier thesis (White, 1969) that individual differences in intelligence and modes of learning may be related, with important consequences for political behavior, will at the least become reopened for investigation.

Future research will in the latter case attempt to determine whether, other things being equal (I realize that they never are), more intelligent children will tend to learn in a more conceptual manner than less intelligent children; whether conceptual as against associative patterns of learning imply a greater understanding and hence mastery of one's environment together with the basis for a greater feeling of personal and political efficacy.

If these connections should hold, they would then qualify Triver's generalization concerning the active role of children in the socialization process: more intelligent children might in general be more active. Whether personal and political confidence ensue will depend, I realize, importantly on environmental factors, in particular on patterns of family rearing as well as parental affection (Coopersmith, 1967). But then these environmental factors will also qualify any sociobiological generalization as well. A neo-Allportian respect for individuality will never be misplaced.

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